Toward a "New School" of surgical research

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In the last issue of the *Canadian Journal of Surgery*, Dr. Ed Harvey highlighted the recent decline in successful grant applications for funding of innovative surgical research.[1] Each era of development in surgery seems to reach a point of congestion from which progress looks impossible. Faced with incrementally smaller gains, its participants resort to self-congratulation: "We are lucky to have lived through this time because such progress will never be seen again." Have we too reached this point? For example, we have used the same immunosuppressants against rejection after transplantation for a quarter of a century and face the prospect of being unable to develop new therapies because 1-year survival rates of 95% leave little room for improvement.[2] Cancer chemotherapy research appears to be trapped in the doldrums where clinical trials of endless recombinations of similar agents prevail. Or consider minimally invasive surgery, where great technological efforts are made to reduce the number of port sites, which, in effect, spares the patient a couple of rapidly healing 5 mm stab wounds. And is this the explanation for the virtual disappearance of preclinical bench research from our academic programs?

To some extent, transplantation has only itself to blame for the current clinical trial inertia. During the growth phase of transplantation, there was too heavy a dependence upon industry to fund trials, and this subverted innovation. For example, early use of tacrolimus showed it to have a more reliable pharmacokinetic profile than cyclosporine, resulting in less rejection after liver transplantation but at the cost of more de novo diabetes. Despite confirmation of these findings in 2 large registration trials 25 years ago, 18 further randomized clinical trials have been done since then with exactly the same results.[3] A review of this problem, sponsored by the Transplantation Society, has suggested that clinical trials should focus on outcomes such as subclinical rejection.[2] The merits or otherwise of using surrogate outcomes has been debated recently in *CJS*.[4,5] It is difficult to imagine that this solution will inspire sufficient enthusiasm to break the logjam.

Science, including the field of surgery, faced this problem of inertia after great progress a century ago. A scientist without the support of academia, industry or government funding made the breakthrough. Albert Einstein, using only mind games, created solutions that we are still learning to fathom today. The saying that he would devote the first 55 minutes of an hour given to solve a problem to determining the proper question is probably apocryphal, but it fits our image of him. The saying may well come from another thinker of the time, John Dewey, who wrote, "It is a familiar and significant saying that a problem well put is half solved."[6] Dewey was part of a group of scholars, many of whom were from the University of Chicago, who formed the New School, which broke the fields of education, social science and economics out of their Victorian straitjackets.

Is it possible then that groups of like-minded surgical scientists could collaborate to forge the proper question, from their patients' point of view? As we think Einstein believed, it will represent 90% of the effort and it will not require the restrictive support of institutions or industry. In doing so we should be mindful of Dewey's remarks that followed his famous quotation:

"Just because a problem well stated is on its way to solution, the determining of a genuine problem is a progressive inquiry; the cases in which a problem and its probable solution flash upon an inquirer are cases where much prior ingestion and digestion have occurred."[6]

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